

TRACTS

Written by the Honourable

Robert Boyle.

Fellow of the R. Society.

O F

*A Discovery of the Admirable RARE-
FACTION of the AIR.*

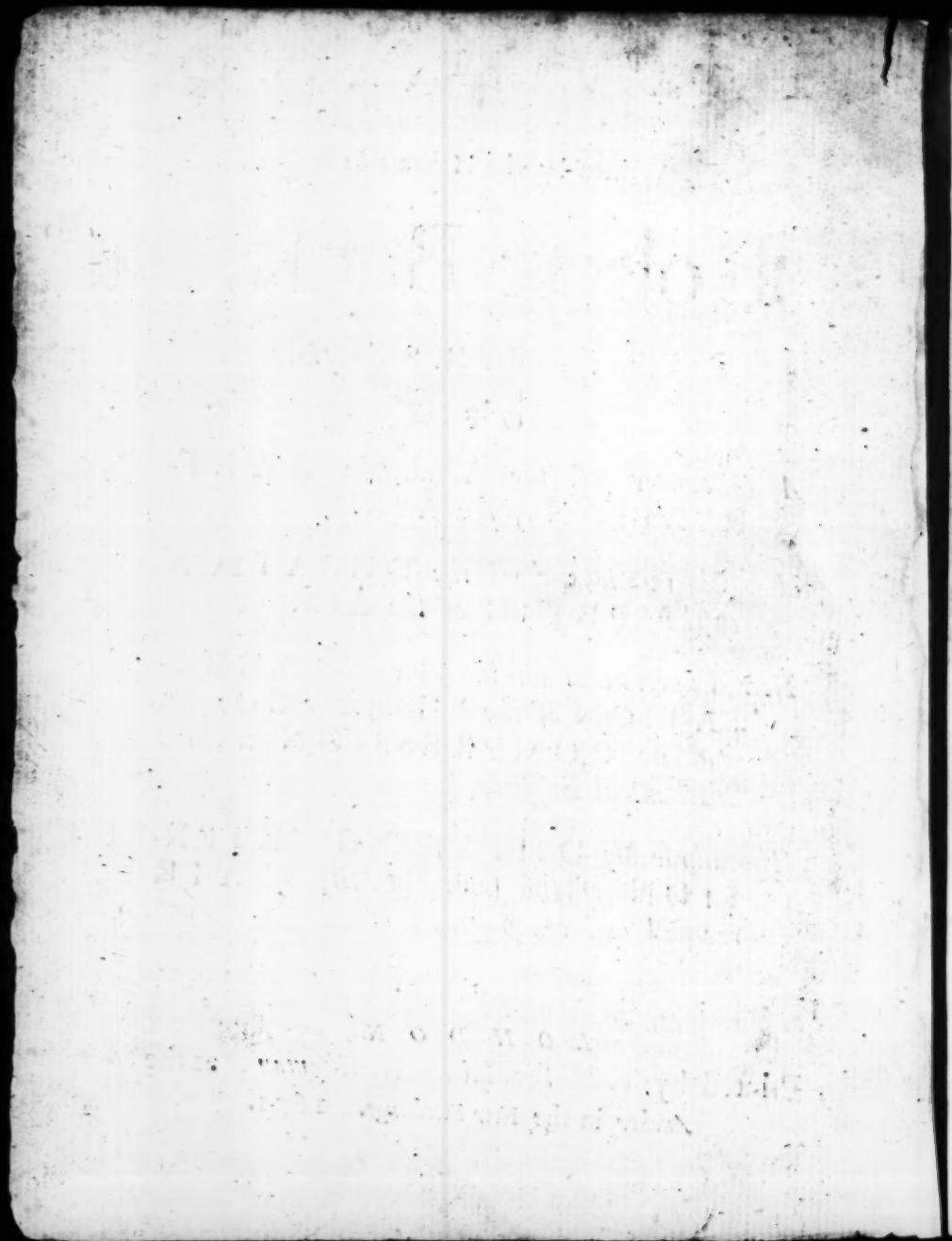
*New Observations about the DURATION
of the SPRING of the AIR.*

*New Experiments touching the CONDEN-
SATION of the AIR by meer COLD;
and its COMPRESSION without
Mechanical Engins.*

*The admirably DIFFERING EXTEN-
SION of the same Quantity of AIR
rarefied and compressed.*

London,

Printed by T. N. for Henry Herringman, at the
Anchor in the New Exchange. 1671.





Advertisement.

THe Author of the following Papers supposeth his Readers to have learned, either from the Books he hath published, or from what hath been borrowed thence by other Writers, the Structure and more familiar Uses of a Pneumatical Engin of his, mentioned by several Authors under the Name of Machina Boyliana; with whose Description therefore those are desired to acquaint themselves, that shall think it worth the while to understand as well as read the following Papers; About which it might be further taken notice

A 2 *of*

of, that the first of them was indeed written
to a Learned Friend (though his Name be
not now annexed for certain reasons;) pre-
sently after which the Three others were
thought fit to be subjoyned. As for the omit-
ting of the Complements and Forms usual at
the close of Epistles, the Author did it as
well to spare the Reader as himself; who
hopes he may be excused, if the Transtitions
from one Discourse to another, and even the
Style and Method of them, be not so
smooth and regular, in regard the ensuing
Writings were traced, when he was afflicted
with a great fit of Sickness, that kept him
from so much as once reading over himself,
what he had indited.

E R R A T A

P. 2. l. 25. r. Air for Fire. p. 9. l. 26. r. great gap. p. 13. l. 18. r. outward for
utmost. p. 19. l. 13. r. laid it round. p. 23. l. 1. r. our for or.



A DISCOVERY
Of the Admirable RAREFACTION
O F
A I R
(even without Heat)

Imparted in a LETTER to a FRIEND.



O not imagine, Sir, that I did at all wonder to see you yesternight so much admire, to hear me talk with so much seeming Extravagancy about the *Rarefaction and Condensation of the Air*; for I confess, that I did deliver something on that occasion, that might easily at first sight appear so near impossible as to be utterly improbable.

And though you were pleas'd even on such an occasion to express a very favorable Opinion of my Veracity, yet thinking it fit, that such an Obligation shculd not divert but engage me, to endeavor to justifie you to your self, by confirming

ing what I said to you; I have already sought and found among Papers, many years since laid aside, some that will enable me to make good more than what the diffidence of my Memory allow'd me to say in the very boldest part of my yesternight's Discourse: For now that I luckily find not only the Origin's of the Relations whereof this Paper contains Copies, but that my Engine is in good order; I am so far qualified to countenance a Discourse, wherein I kept somewhat within compass, that though it will perhaps cost me much pains and trouble to make *ex tempore* Experiments fully equal to the enclos'd; yet if any just doubt should require it, I presume I can make Ocular Proof, of at least as much as I last night told you.

And now 'tis time, after havin^y, contrary to my Custom, rais'd in you a high expectation, that I endeavor in some measure to answer it, which I hope I shall the more easily do, because the agreement, you have often had occasion to observe between the Relations registered in my *Adversaria*, and the *Phænomena* of the Experiments they describe, will, I presume, make it needless to perswade you that the ensuing Trial's, being transcribed thence, may be safely credited. Wherefore I shall proceed to annex them, as soon as I have premised a few Historical Lines by way of Manuduction to them.

'Tis now many years since, that having a desire to reduce the Fire to a degree of Rarefaction that appear'd to be considerable, upon surer grounds than slight Conjectures, I attempted to do it by the help of Heat, and particularly by that of an *Aeolipile*, which I have mentioned in another Tract; but finding that the diligent *Mersennus* had, if there be no mistake in his account, been able to rarifie Air that way, full as much or more than I could, I betook me to try, whether I could not by the Spring of the Air (without Heat) procure a greater expansion of it? I found (as I have long since elsewhere related) that in the Pneumatical Engin, which has been since called the *Machina Boyleiana*, I could encrease the

New
Physico-
Mechani-
cal Expe-
riments.

Exper. &c.

Ibidem.

the Expansion of the Air, till the Body attain'd to about 152 times its former and usual Dimensions. But this Expansion, though it were above twice as great as the utmost procure by *Mersennus*, did not yet satisfie me, but put me (according to what I there intimate) upon another Contrivance; which though put in practice eight or nine years ago, (as the date of one of the Trials may inform you,) had the relation of its Successes laid aside among those of others, made in the same Engin which yet lie by me unpublish'd. So that I may now proceed to give you the Transcripts of the Trials themselves, as they were hastily and inelegantly, but very faithfully, set down among my *Pneumatical Collections*. And this I am ready to do, as soon as I shall have intimated to you, that in that Nob'e Collection of Experiments that has about two years since appeared in Publick, as the First-fruits of the justly famous *Florentine Academy*, I find, that those *Virtuosi* had according to their sagacity, so advanc'd the Extent of the Air, as without the he'p of Heat to bring the Dilatation to exceed 173 times its former Dimensions; and that which made their Improvement the more considerable, and consequently the more worthy of them, is, that they procured this great Rarefaction, as well as I had done mine, by the Air's own Spring; and had surpas'd without the help of my Engin, what I was then at first able to do by the Conveniences that it afforded me. Whereupon rememb'reing what I had perform'd in that kind several years before, I sought among my Papers for the Trials I had then made, and found those Notes, whereof I now at length think it high time to give you the promis'd Copies in the following Terms.

Experiment I.

WE took a round Glass-Egg (as they call them) of clear mettal and furnished with a Pipe or Shank of some Inches in length ; this we fill'd with water, and convey'd both it, and a Viol with Water in it, into a Receiver of a convenient size , and by pumping the Air out of it , we made the Bubbles both in the Egg and the Viol to disclose themselves in great numbers ; so as to make the Liquor in the Glass-Egg feem to boil , and to make all that was in the Shank really to run over. When we thought the Water was sufficiently freed from Air, which it was not quickly brought to be , we took out the Glasses and fill'd up the Pipe of the Egg with Water taken out of the Viol , and inverted it into more of the same Water , in such manner that the Egg was quite full, Shank and all , excepting a small Bubble of Air that we purposely left to gain the top of the Egg ; where the Glass being transparent , with a pair of Compasfes we measured as accurately as we could , and found it to be a Tenth and les than two Centefms of an Inch. Then putting the Glasses again into the Receiver , we set the Pump a work , and the little Bubble after a while began to expand it self , which when it had once done , it did at each suck strangely increase, till at length it drove all the Water out of the round part of the Glass. And least it might be objected , That 'twas only the subsiding of the Water upon the with-drawing of the outward Air, that before kept it up to the Top of the Glass, we caus'd the pumping to be so continued , till the expanded Air had several times driven the Water in the Pipe of the Egg a pretty way beneath the level of the external and surrounding Water in the other Glais. This done, we let in the Air by degrees , with a design to observe, what Bubble we Should find at the Top of the Egg , when the Water should
be

be again driven up into its Cavity. But the expanded Air had forced over so much Water, that there remain'd not enough to fill the Globulous part of the Egge: Wherefore we tried the Experiment again, and when we had proceeded thus far, we compar'd the above-mention'd Diameter of the small Bubble, with that of the Spherical part of the Glafs, which we took with a pair of *Callaper Compasses*; and though we found it to be somewhat moie than 20 times as great, yet being willing rather to disfavour than flatter the Experiment, we suppos'd the two Diameters to be as 1 to 20, and consequently, since as *Euclid* demonstrates, the Proportion between Spheres is triplicate to that of their Diameters, and in our Case the Cube of the lesser Diameter being 1, is also ^{El. 12.} *Propos.* ^{ult.}

but 1, the Cube of 20 the greater Diameter must be 8000; and so the Air appears to have, by expanding it self, acquir'd a place 8000 times as big as it possest before. Nor was it overseen by us, that the Globulous part of such Glasses as we us'd is scarce ever made Spherical. But not only I, but Dr. *Wallis*, who was pleas'd to assist at the Experiment, concluded, that the Cavity of the Shank, which the expanded Air drove the Water from, but which we did not compute, would make abundant Compensation for the two above-mentioned particulars. After this, for further satisfaction, we took water, laboriously freed from Air, and putting it into the same Glass-Egg, we inverted it as before, but left not any Bubble in it. This we did, that, in case we could make the Water subside, the Experiment might prevent a suspicion, that some Air latitant in the Water might increase the Bubble that was formerly left in it; having then exhausted the Receiver as much as before, and, if we mistook not, more, the Water in the Egg did not at all subside; but at length, with obstinat pumping, a Bubble disclos'd it self, and drove all the Water clear out of the round part of the Glass; and, though by reason of some small Leaks that we could

not

not find or stop, we were not able, as before, to make the expanded Air depress the Water in the Shank beneath the surface of the external Water, yet we wanted very little of it; and then out of weariness giving over, we found, that when the Water was impell'd up again into the Egg, there was at the top of it a Bubble, whose Diameter we measur'd as faithfully as we could, and found it to be to the Diameter of the Globular part of the Glass as 1 to 14; so that, though the little Bubble had been a perfect Sphere, yet Spheres being, as was lately noted, in triplicate proportions to their Diameters, the Bubble when expanded, must have been 2744 times as big as the Bubble unexpanded. But Dr. Wallis, who will be allowed to be a very competent Judge in these matters, observing (what we all took notice of) the great thinness of the Bubble, positively and constantly affirm'd, that he could not estimate it to be at most any bigger than the third part of a perfect Sphere of that Diameter; by which estimate the expansion of the Bubble must have reach'd to 8232 times its natural Dimensions.

NB. By letting in Water into the Receiver as much as it would admit, we found, that, by reason of some secret Leak, we had not been able so to exhaust it, but that there remain'd some Air.

Experiment II.

June 2.
1662.

A Small and almost inconspicuous Bubble expanded it self, when the ambient Air was pretty well exhausted, to more than 10000 times its former extent. The manner thus; We took a small Bolt-head, blown by a Lamp, which contain'd in all about 80 Grains of Water, and inverting the small Neck into a Jar of Water, it was included in the Receiver, and the ambient Air being exhausted, store of Bubbles rose out of the Water, and expanding it self, quickly

quickly drove all the Water out of the Bolt-head. Then re-admitting the outward Air, the Bolt-head was presently almost fill'd, and all the expanded Air shrank into a Bubble little bigger than a small Pins head; then taking the Bolt-head out of the Water, and inverting it, that the Bubble might get out at the Neck, we carefully fill'd it up with the Water that had been freed from Air, and then inverting it as before into the Jar with Water, we again inclipted it, and, after some exuctions, found, that therewas gotten out of the Water into the Neck a very conspicuous Bubble, which upon the admitting of the Air, shrank almost into an invisible one, and ascended into the head of the Glass. Then again exhausting the Receiver very well, we found it expand it self so as to fill all the Capacity of the Bolt-head, and to drive out almost all the Water. And upon the re-admitting of the Air, it again shrank into a Bubble whose Diameter (according to our best estimate) was not bigger than one two and twentieth part of the Diameter of the Head of the above-mention'd Glass; so that to fill the whole Cavity of the Head only, it expanded it self 10648 times: but because it fill'd likewise the greatest part of the Neck, we found by weighing the Water that fill'd that part, and the Water that fill'd the Head, that the Capacity of that part of the Neck, was almost a third of the Capacity of the Head, being as 141 to 481: if therefore 481, the Capacity of the Head, contain'd it 10648 times; 141, the Capacity of the Neck, must contain it $312\frac{1}{3}$ times; so that, in all, the small Bubble of Air was expanded to above 13769 times its former Bulke.

The Diameter of the small Bubble retracted was $\frac{1}{27}$ of an Inch.

The Diameter of the out-side of the Head of the Glass, was $\frac{22}{27}$ of an Inch.

The Water that fill'd the Head only weigh'd 60 Grains.

The

The Water that fill'd the Head and as much of the Neck as the Air had before expanded it self into , weighed 78 $\frac{1}{2}$ Grains ; so that that part of the Neck weigh'd 17 $\frac{1}{2}$ Grains.

The Bolt-head it self weigh'd 15 Grains.

I might have set down this Second Experiment un-accompanied either with the First, or with that I am going to subjoyn ; because the expansion produced by neither of them was, at least by measure , so vast , as that produc'd by the Trial newly mention'd : but this was so stupendious , that I thought it not so fit to present it to you by it self alone , but rather accompani'd with other Experiments , the least prosprous of which produc'd a Dilatation of Air sufficient for my present purpose , and such as may not a little confirm , That what is recited in the Second Experiment , was neither a lucky chance , or mistake . And that may be enough for my present purpose ; for as for the little Abatements , that some will perhaps think fit to be made upon the score of the unequal thickness of Glass or some such Circumstances , they are not considerable enough to deserve to be now solicitously debated , nor to hinder the expansion that must be granted from proving what they are alledg'd for : wherefore I will proceed to what follows .

Experiment III.

WE tried this Experiment again , and found a small Bubble , much about $\frac{1}{2}$ of an Inch in Diameter , fill'd not only the Ball at the end of the Bolt-head (which was 1 $\frac{1}{2}$ of an Inch in Diameter) but the whole Neck , which contain'd near as much Water as the Head , and beat down the surface of the Water within the Pipe much below that of the Water without the Pipe ,

These

These Experiments already found among my old Papers, will, I hope, without my seeking for more, suffice to manifest, that the expansion which the Air may be reduc'd to without heat is indeed *Admirable*; for if we make an estimate of it but according to the Experiment which had the most moderate success, it appear'd, that one space possess'd, though not adequately fill'd, by a portion of Air, may have its Air extended to at least 2744 spaces equal to it; I say, *at least*, because very probably it was above twice as great: And if we make our estimate according to the most prosperous of our Trials, we must allow the Air to be rarefiable at least 13000 times (I say again *at least*) because I am not sure, that in that Tryal it was reduc'd (not fully, though perhaps very near) to the uttermost degree of rarefaction attainable in our Engin: so that I presume you will now grant, that I spoke warily and much within Compals, when I mention'd but an expansion from one to a thousand.

And now having perform'd the promise I made you, it remains only that I take notice of the request that you made me, about communicating these Experiments to the Curious. But this desire of yours is oppos'd by no small inconveniencies that would resist my complyance with it. For it would oblige me by tearing out these Papers, to dis-member a Collection long ago in making, and wherein they were plac'd to be much otherwise dispos'd of, and not only make a greas gap in it, but strip or deprive it of some things that were the likeliest to re-command it. Besides, that these appearing before the rest, are odd enough to make these seem far less uncommon than perhaps otherwise they would. Yet all this notwithstanding, I find it uneasy to refuse what you and those Friends that concur with you, on this occasion, desire, that if after having once more perus'd these Papers, you persist in the same earnestness you express'd yesterday

(when you had not yet seen them) I shall not refuse you the Disposal of them, both for the Reason now given, and because I have been inform'd as well by you as by other means, That the Rarefaction of the Air is at present the subject that busies the Disquisitions of several eminent *Virtuosi*, both Domestick and Foreign, to whom I pay so much respect, that I shall think it a happiness, if it may be acceptable to them, not only because it will be seasonable, but because, that though the Engin, that most of the Attempts were made in, has not been thought altogether Barren, yet these Trials will probably pass for one of the least Inconsiderable Productions of it: And these two Services I hope this short Writing may do several Ingenious Readers; the one, that it will invite and accustom them to take notice of and consider more than most, even of the most Learned, are wont to do, the great Subtlety of Nature, and the scarce imaginable smallness of those Aerial Instruments, that she employs even about visible Operations: The other, that these Relations will excite the more Curious and piercing Wits to debate, and I hope help them to solve the two Problems here propos'd to them; *What Figures and Motions may be assign'd to the Particles of the Air, to explicate its so wonderful Rarefianleness, and that perhaps without quite losing its Durable Spring;* and *How the Air comes to be rarefiable so many times more without Heat, than hitherto we have found it to be by Heat.* To which might be added, as a Third, *What might be reasonably conjectur'd about that part of the Cavity of an exactly clos'd Glass, Where, though the Eye discovers no visible substance harbour'd in it, it appears not that the common Air does adæquately fill so much as the Ten thousandth part?*

N E W.



N E W
 OBSERVATIONS
 About the
 DURATION of the SPRING
 of EXPANDED AIR,
 (Subjoyn'd by way of APPENDIX
 to the fore-going EPISTLE.)

Forasmuch as re-viewing the former Paper about the *Rarefaction of the Air*, I took notice in the Close of it of an Expression (*viz.* *And that perhaps without quite losing its Durable Spring*) which I fear may to some Readers seem to need Explication; it will not be improper on this occasion to subjoyn something by way of Appendix about it.

First then, the Reason why in this short Intimation I thought fit to employ the diffident Term *perhaps*, was, because really I had not (nor yet have) been taught by Trial, Whether and how far the utmost expansion of the Air actuallly produc'd in my Engin, or otherwise procurable, and its retaining a sensible Sp. ing, are consistent? I express my self

thus, to insinuate, that I thought of other Instruments and Methods whereby the Dilatation of the Air may not improbably be measur'd and promoted; as by making the Torricellian Experiment in a Glass with a very capacious Head or Globulous part, and applying the Aerial Particles that will ascend out of the subsiding Mercury, together with a Bubble of other Air, if it be needful to the use we have been speaking. Something also may be done to some purpose with very fine and large Fish-bladders; but I shall not insist on these or the other Expedients that came into my thoughts, contenting my self to have intimated and thereby acknowledged, that there may be other means besides the *Machina Boyleiana*, to bring Air to a very great Expansion. But whether any of them will surpass what has been actually attain'd in that Engin, time must declare; till when, we shall be content to make use of the Experiments it has already actually furnish'd us with.

Wherefore to come to the second or other remaining part of it, whereas in the mentioning of the Spring of the Expanded Air, I employ'd the Attribute of *Durable*, you may easily gather the Reason from what I am now going to annex.

I had observ'd, not without some wonder, in the Inquirers into the Nature of the Air, that they have not, that I know of, so much as attempted to discover, Whether the Air either in the utmost or in the intermediate degrees we can bring it to, does retain a constant and durable Elasticity?

For first, Men have not yet determin'd, Whether a portion of our common Air being exactly shut up in an Hermetically seal'd Glass, or some other exactly clos'd Vessel, will constantly and uniformly, for a moderate time at least, retain the degree of Elasticity it had when 'twas shut up? And whether it will not sometimes vary its pressure, as we see that the Atmospherical (though I think upon peculiar grounds)

grounds) is by the help of our Baroscopes observ'd to do? Next it does not appear, Whether included Air, in case it retain an uniform Elasticity for a moderate time, will retain it for a very long one? Nay, Whether it would not at length come not only to have a weaker Spring, but perhaps to have no sensible Spring at all? as we see it happen in Sword-Blades and divers other Springy Bodies, which after having stood too long bent, will continue so, and lose their former power of Self-restitution, as they call it.

Thirdly, Men have not yet determin'd any thing about the degrees of the Airs Elasticity, Whether the Durableness and Uniformity, or Varying of its Strength, may not depend upon the differing degree, it had when 'twas first shut up?

Fourthly, Much less have we yet attempted to discover, Whether the Spring of an inclos'd portion of Air may be sometimes weaken'd, and sometimes strengthen'd by the Changes, as to Gravity, of the utmost Atmospherical Air, the New and Full Moon? To which I might add divers other external Accidens, which as yet we scarce suspect. And to these I might add some other Doubts and Inquiries, that may not be impertinently suggested, but here would, I fear, pass for a Digression.

Wherefore I shall proceed to tell you, that having taken notice of it, as an Omission among the Inquirers into the nature of the Air, in whose negligence I was too long a sharer, that we have not, that I know of, so much as attempted to discover this it self: Whether the Air either in the utmost or in the intermediate Degrees of Rarefaction we might bring it to, would for a considerably long time retain its Elasticity, or at least some determinate degree of it, or lose it by determinate and regular Decrementes, I thought fit to make some Trials about this matter, but cannot brag of the success of my Intentions, having been hindred either by

want

want of Instruments, or by Removes, or by Sickness, or by unlucky Accidents, or by one unwelcome thing or other, from accomplishing what I had chiefly designed, and partly also made some progress in it; but yet to give you some hints, as well as some occasion to more prosperous Experiments, I shall not stick to annex what I readily call to mind about my attempts on that occasion.

I remember then, that when I first began to try something in order to my design, being destitute of fit Accommodations, I was fain to content my self, by causing a good Bubble of Glass with a Stem to be so blown at the flame of a Lamp, that whilst the Ball was yet exceeding hot, and consequently contain'd none but highly rarefi'd Air, the Stem was very nimbly clapt into the flame of a Candle that was purposely kept ready at hand; so that being slender, it was in a trice seal'd up, and the Air within remain'd as much expand'd as the great heat, it had been expos'd to, had brought it to be. This Bubble many Moneths after, I inverted into a Basin of Water, and having broken off the Seal under the Surface of it, the Liquor was violently impell'd into the Cavity, but yet was not able to fill it, a considerable part being defended from the further ascension of the Water by the Spring of the remaining Air, which for all the long stretch it had been put to, had not lost any thing of its Spring that we could take notice of. But this was a Trial, in which I could by no means acquiesce; and therefore when I was a little more be-friended by opportunity, I try'd another way, partly to give a somewhat pleasing surprize to unaccustom'd Beholders, and partly, because though it could not shew all that I desir'd, yet it might plainly shew, that the Air, even at a very considerable Extension, would hold out for a considerable time. Wherefore leaving a very small proportion of Air in the Folds of a fine limber Bladder, whose Neck was very closely tied, I caus'd it to be, by the help of the *Ma-china*

china Boyliana, so expanded, that at length it so dilated it self, as to seem to fill the whole Bladder, and reduce it to the extent it had just before 'twas empty'd; and the Bladder by a peculiar contrivance was so included in another Vessel, that being protected from all intrusion of the outward Air, it maintain'd its plump and tumid Figure, and in that unwrinkled state I shew'd it, *many Moneths since*, to some *Virtuosi*, (now here in *London*) after it had continued so, if I mistake not, near two year. Since the writing of this, I did at length find the newly mention'd Vessel, and shew'd it to some curious Spectators, who with me took notice, that the included Bladder, instead of being wrinkle or shrunk, appear'd to be plump and full, as well blown Bladders are wont to be: so that *many Moneths* (perhaps a dozen) may be added to the freshly mention'd Duration of the Expanded Air.

But this way satisfying me neither as to some of the Particulars I desir'd my Attempts should discover, I devis'd a little Instrument, whose Contrivance though it seem'd very simple, promis'd, and for some time gave me a far more accurate account of what I expected. The Instrument, if you desire it, I can easily shew you, having lately been forc'd to make a new one, which is now by me; but it may suffice to tell you, that 'tis so fram'd; that 'tis fit to discover, besides divers other things, Whether and how long Air brought to the greatest Expansion I could conveniently reduce it to in my Engin, will retain its Spring? And by what degrees or stages and periods of time, the decrement, if any be, is made? But of the issue of the Trial made in it, I can give you but a very imperfect Account, in regard that, though I made it about three years ago, yet having left the Instrument in a place where 'tis so lodg'd, that I cannot have it without returning thither, till I see it again my self, I dare not venture to judge of the success of the Experiment; only this I remember,

member, that I took no notice of any observable diminution in the Airs Elasticity, though it were press'd, and, as it were, clogg'd with a weight, that one would wonder how it could, when 'twas so highly rarefy'd, support for one

* See the minute *

*Post-
script.*

There is also another way that I contriv'd, wherein the Air in a little portable Instrument, which I can shew you, being expanded, as one may guess, to 5 or 600 times (perhaps 1000 times) its wonted extent, has not only for a long time preserv'd its Spring, but satisfies me also about one of my chief Queries, which was, Whether the Air very much dilated without Heat, would be considerably sensible of external Heat? Which it plainly appears to be in this Instrument; where, notwithstanding the great Rarity, it has already attain'd and seems likely to preserve, the Heat of ones hand applid to the outside (of the Vessel) has a quick and very manifest Operation; and upon the with-drawing of it, the sensible Air quickly returns to its former Dimensions, as well as temper, so that one may employ it as a kind of Weather-Glass, and perhaps make some discoveries by long comparing it therewith.

But hitherto I have been doing, what I do not love to do, and very rarely have done when I mention my own Experiments, that is, I have not punctually specified any determinate quantities and proportions of the things spoken of; but one of my former Trial's I have newly found out, registred in a loose Note, and therefore the Quantities being annex'd, I hope it may both give some countenance to what I have been saying, and give you some, though not an entire satisfaction about the thing it self.

March 18. A Glafs as Cylindrical as we could get it blown at our Lamp, and having a long Stem coming out at the unseal'd end, was quite fill'd with Water and inverted into Water placed at the bottom of a large Pipe seal'd at one end, and

of

of 3 or 4 foot in length. This external Pipe, so called for distinction sake, was exhausted, till the Air, that disclos'd it self in the Water of the internal Pipe, had drawn out the Water iu the Cylindrical Pipe, as low as the upper part of the Stem; at which great Expansion of the Air, the external Pipe being speedily and securely clos'd by a certain contrivance; the Air thus rarefi'd was kept sometimes in my own Chamber that was warmer, sometimes in an under-room; and after it had been kept from first to last about 11 Weeks or 3 Moneths, if I mis-remember not, without any other remarkable Variation, than that in the cold room, the Water ascended, as I guess'd, about $\frac{1}{4}$, or near $\frac{1}{4}$, at that part of the internal Pipe where the lower end of the Cylinder gradually lessen'd it self into the slender Stem. Yesterday I invited Doctor *Wallis* to be present at the breaking of the Glass, and to favour me with his assistance, for the better estimating the Expansion of the Air upon the breaking of the clos'd Apex. The Water was but leasurely (because of the slenderness of the Orifice that was made for the Air to get into it) impell'd up into the formerly deserted Cavity of the Cylinder, which it fill'd all, save a little Bubble which was exceeding shallow. We made use of our Eyes at a fit distance, and of Compasses both ordinary and Calliper, to obtain these measures. The Cylindrical part of the internal Pipe was 3 Inches in length and $\frac{3}{4}$ of an Inch (or less in Diameter on the outside.) The Bubble was $\frac{1}{16}$ in Diameter and about 2 Centesims in depth: From all which, by the Doctors Calculation, the Bubble, to the space it possest unexpanded, was as 1 to 1350.

W W W W W W W W W W W W

New Experiments

Touching the CONDENSATION
Of the AIR by meer COLD,
And its Compression without Mechanical Engins.

Because 'tis as truly as commonly said, that *Contraria juxta se posita magis elucent*, and because what I am now going to interpose, is little less than necessary to be premised, to clear the way to what follows, and to connect the past writing to that which is to ensue; it will not be improper to add something in this place touching the *Condensation and Compression of the Air*.

And here I cannot but a little wonder, that among so many, that have had occasion to consider the nature of Cold, and the Condensation of the Air by it, I have not yet met with any that have had the curiosity to measure that Condensation; wherefore I long since attempted to do it, as I have related in another discourse; but not having that by me at present, and rememb'ring in general, that I did it in Winter, when it may be objected, that the Air being al-ready præ-affected with the coldness of the season, was not capable of being so considerably contracted by an additional cold, as it would be at a time of year, when it is wont to be in a state of greater laxity; I thought fit to make the Experiment about the beginning of *Autumn*, without tying my self to make it with the same circumstances that I had done before; the Event of this Trial I find registered as follows.

After

[After the midst of September, on a Sun-shiny day, and about Noon (which circumstances we made choice of, that the Air might be the more rare and expanded) we took a Bolt-head or round Viol furnished with a long Stem, and plac'd it in a Frame purposely provided, so that the Stem was perpendicular to the Horizon, and the globous part was supported by such a Vessel, that thorow a hole, purposely made at its middle, the Shank reach'd downwards, till the Orifice of it was a little immers'd beneath the Surface of a Glas full of Water, that was plac'd at the bottom of the Frame. This done, we took a good proportion of Ice, and having beaten it in a Mortar, and mixt it with a due quantity of Bay-salt, we not only laid round about the lower part of the Ball, but the Vessel, contiguous to that part, being purposely made with turnd-up brims, we were enabled to heap up the frigorifick Mixture, so as to bury the whole globulous part of the Glas in it, and cover the very top of it therewith to a considerable thickness; upon which occasion the Air within being exceedingly refrigerated, the Water, into which the Shank terminated, was made to ascend somewhat fast along the Cavity of that Shank, till we perceiv'd it would reach no higher; but after a while began to subside again; which nick of time being carefully watch'd, we made a Mark at the highest Station of the Water, and then taking out the Bolt-head, we fill'd it with Water, making an allowance for that small part of the Stem which was immers'd at the beginning of the Operation. This Water we weighed, and found it amount to nineteen Ounces and six Drachms; then weighing as much Water, as suffic'd to fill the Shank up to the Mark newly mention'd, we found that to be one Ounce and three Drachms; by which number the former being divided, the Quotient was $14\frac{1}{4}$ Drachms; so that the proportion of the two quantities of Water being as 11 to 158, the space into which the Air was condensed by

Refrigeration , was to the space it posses'd in its former state of laxity as 147 to 158 , and consequently the greatest Condensation, that such a time of the year and in such Weather, so high a Refrigeration could bring the Air to, made it lose but $\frac{1}{11}$ of its former extent.]

NB. First , The Stem of the Glass ought to be of a considerable length , least by the great contraction, made of the Air in the Ball by its high refrigeration, the Water should ascend into the Cavity of the Ball it self, and thereby become exceeding difficult to be measur'd.

Secondly , If one would be nice, one may take notice, that the height to which the Water ascended in the Stem, was about two foot ; which Cylinder of Water, by its weight or tendency downward , might somewhat hinder the Liquor from ascending quite so high as it would , and consequently keep the Condensation of the Air from appearing fully so great as it was, but so light a Cylinder as that of the suspended Water, would scarce be very considerable.

Thirdly , When the Water was ascended near as high in the Shank as it would rise, there was observ'd in it an odd kind of *Subsultus*, or rising and falling alternatively , almost like the *Mercury* in the *Torriceilian Experiment* , before the *Mercury* comes to settle after its first subsidence. [But the consideration of this *Phænomenon* belongs not to this place ; for which reason I insist not on this , and forbear mentioning some others.]

Fourthly , That though it appears not by this Experiment , whether the Cold thus produced is equal to that of Frosty Weather in Winter , and consequently capable of contracting the Air as much as that season is wont to do; yet by preceding Trials made with fit Instruments , I had found that by such an application of Ice and Salt as we had made in

in the late Experiment , a greater degree of Cold , and that in a warmer season , might be produced , than had been found necessary to make Frosty Weather in Winter: The way of Experimenting for brevities sake I omit , but if you please you may command it .]

But 'tis not chiefly to acquaint you with the Condensation that Nature uses to make of Air , that I have been entertaining you with these Memorials ; for that which makes it very pertinent to my present purpose is , that it will shew you , that as to the Condensation or Compression of Air that I am to recite , though Cold were employ'd about it , yet it was not really produced by Cold , which could not contract the Air to so much as half that degree , you will find it was reduc'd to by our Operation (prefently to be mentioned ;) wherein the frigorifick mixture did not primarily or immediately compress the included Air , but only so affected the Water that was shut up with it in the same Vessel , as to make it swell , and consequently crowd the Aerial Particles into less room : Wherefore it now remains that we proceed to the Experiment it self ; a short Account of which , be pleas'd to take in the ensuing Transcript .

[To convince some Strangers , we took a new Glass Bolt-head , with a Neck not long , and fill'd it so far with Common Water , that being Hermetically seal'd , the Liquor reach'd within three Inches of the Top , as near as we could guess , by measuring with a Ruler , and making an Estimate of the sharp end , made so for the conveniency of sealing up the Glass ; which sharp end we guess'd to be about $\frac{1}{4}$ of an Inch in length ; then applying Snow and Salt to the lower part of the Bolt-head , we readily drove out the Water further and further into the Neck , till at length it was got up to the Basis of the sharp and conical end , where the Glass was seal'd ; and then just as I was looking upon it , the Glass flew with a noise about my Ears , being broke into many pieces ,

pieces, which argued the Compression of the Air to have been very great. And Doctor *Wallis*, who was present, and measur'd it from time to time, desired me to register the Experiment, with his Estimate of the Compression, which was, that the Air was reduc'd into the 40th part of its former Extension.]

I know so great a Condensation of Air will seem strange to those, that have taken notice, that some of the best Mathematicians of our Age, that have made use of Wind-Guns and other forcible Engins, to crowd the Air into as narrow room as possibly they could, confess themselves not to have been able with all their strength and industry to force the Air into less than the 15th part of its usual extent; and *See Mer-
sen. Phen.
Pneum.* besides, that this was done in Countreys, where the Air may well be suppos'd more lame and rare than in England. I Prop. 32. confess, I saw no Trials made with Wind-Guns, that convinced me that the Condensation was so great as that newly specified: (about which *Mersen*, himself somewhat hæsitates, seeming to doubt, whether the Air were indeed restrain'd into a 15th, or but into one 8th part of its former room.) And he that hath observ'd and consider'd, as I have done, that in Wind-Fountains (as they call them) of Glass, the Air will seem to be notably compress'd, whilst indeed we could not find it compress'd into much less than its third part, will be the less unapt to be diffident of the great things that are said of the Compression of the Air; but because Experience has informed us that our English Air may in peculiar Instruments be forcibly crowded into a 10th, 12th, or perhaps a 15th part of its former extent, I am content to take it for granted, what is related about the Compression of the Air into the 15th part of its usual dimensions: and yet our Experiment will be a *considerabler* instance of the great compressibility, if I may so speak, of the Air; for according to the

the Estimate delivered in the fore-going Narrative, or Compression, which was without Mechanical Instruments or Engines, reduc'd the Air into the 40th part of the space it had lately posseſſ'd; and how great a force is requisite, when the Air is once considerably condens'd, to ſurmount, though but a little, its great reſiſtance to further Condensation, may be gathered from the Observations about the gradual renitency of the Air to Compression, which we many years ſince made with *Mercury*, and afterwards published in another Treatife: but though, upon the recited grounds, that great Compression of the Air produc'd by our Experiment, my, as I was ſaying, ſeem very *ſtrange*, yet it would not ſeem *incredible*, if I ſhould here borrow thoſe Experiments and Observations from my already publish'd *History*, and ſome *unpublifiſh Papers* about *Cold*, that would countenance what I have been delivering; and especially if I ſhould stay to communicate to you the way, I not unſuccesſfully made uſe of, to estimate by *weight* the great force of the expansion of Water upon its freezing. But ſince an account of this contrivance is not here neceſſary, and would require more leaſure than I can ſpare at this time; it remains only, that by way of Corollaries from what has been hitherto delivered in this and the two precedent Writings, we rather point at, than discourse of, ſome Observations that it ſuggested to us, in the ensuing Paper.

*Defence
against
Linus.*

Of



Of the Admirably Differing
EXTENSION
 Of the same Quantity of
A I R
 RAREFIED and COMPRESSED.

HAVING already declared, that what I pretend to in the Close, is but to set down some Observations, that result from, or are suggested by what hath been already delivered, I presume I need not trouble you or my self, with any other *Preface* to what follows.

That then which seems first worth taking notice of, is the differing alterations that the Air is subjected to by Cold and Heat; For whereas we could not find in this our Climate, that Cold would reduce the Air into near the 20th part of its former Extension by Condensation; Heat would advance it to near 70 times its usual Laxity by Rarefaction.

Next,

Next, We may observe, That by Engins and other Artificial Instruments, the Air may be two or three times as much compress'd, as Nature is wont to condense it by Cold, even in Frosty Weather; and so on the other side, the Air may by the intervention of Art and Instruments be much more rarefied and expanded, than it has been yet found to be by the bare application of External Heat, though it were that of an intense Fire it self.

Furthermore, It may seem worth while, to observe, how See, &c. to
theutmost
Expans-
on by
Heat,
M. tenn.
in Cog.
Phyl.
Mathem. much the utmost degree of Rarefaction by Heat, that Experiment has shewed us of the Air, falls short of the Degree of Expansion, to which it has been advanced in our Pneumatical Engine, the proportion betwixt these two Expansions being that of 1 to 70, or thereabout

But perhaps it will not be necessary to conclude, That the Air is so much more rarefiable than compressible, as most Readers will be prone to infer, by comparing the greatest Compression and Expansion of it, that are mentioned in these Experiments; since, if I mistake not, it ought to be considered, that the Air, we made our Trials with, upon the surface of the Earth, was not (no more than is the Air we commonly breath) properly in a true natural Confinement as they speak, or, if you please, in a free and indifferent state in reference to Rarefaction and Condensation, but was already highly compress'd by the weight of the Atmospheric Pillar that lean'd upon it: so that it had already a very strong tenacity to further Compression; whereas the Air that was to be rarefied, had by vertue of its Spring (strongly bent by the weight of the Incumbent Air) a strong Propension or tendency to dilate it self; which difference I must content my self to have intimated, and leave you to consider, Whether and how much it may alter the Case.

Fourthly, To some perhaps it wi'l seem more fit to consider, than easie to resolve, how, since the Corpuscles of the Air are acknowledg'd to be heavy, and those that remain, must be so wonderful thinly dispeſt, in the Cavity of the Receiver they come to be supported, and kept as it were swimming therein, and do not appear to ſubſide by their own weight, the *Materia ſubtilis* (though the preſence of that ſhould be admitted) not appearing to have gravity, wherewith to ſustain them; and the *Vacuum* (if that be ſuppoſ'd wherever the Aerial Particles are not) being too near a-kinne to nothing, to be able to opoſe their deſcent: but though ſomething may be ſuggeſted about the ſolution of this doubt, my haſte obliges me to leave it ſuch.

Fifthly, I will not make it my busineſs to make mention, in this place, of the wonder that may be *justly* excited in those, that, when they look on one of our well exhausted Receivers, attentively conſider, how ſmall a proportion, the common Aerial Corpuscles, which are very sparingly diſpers'd there, bear to the whole Cavity of the Vessel, which, before it was exhausted, was thought to be repleniſht with Air alone. This I ſay, I ſhall not ſollicitouſly obſerve, because I think I need not; for I little doubt, the thing will be obſerv'd and laid hold off, both by the Carteſians and Epicureans; the former of which will endeavour thereby to eſtabliſh the neceſſity of their *Materia ſubtilis*, to maintain the Plenitude of the World, and the Circle they attribute to Moving Bodies; and the later will here triumphantly pretend to have a more illuſtrious iſtance, than ever, of their *Vacuum coacerwatum* within the World, ſince here is an impene- trable Vessel, out of which 'tis maniſt that an almoſt incre- dible proportion of Aerial ſubſtance hath been maniſtly made to iſſue; whereas 'tis no wayes maniſt to any of our Senses, that any other body has got in to ſucceed in its room: Wherefore leaving them to debate, what it is that is contain'd

in

in that far greatest part of the Vessel, that the Air pump't out of our Receiver has deserted, I take notice,

Sixthly, That, to conclude with what was the main drift of this and the fore-going Papers, we are here invited to observe with wonder the stupendious mutability of the Air, as to rarity and density, whereby the same quantity of Air being sometimes compress'd, sometimes dilated, may change its dimensions to a degree, that seems almost to transcend the power of Nature and Art; and by consequence might probably be rejected as incredible, if it were abruptly and nakedly propos'd: and therefore it will be convenient to do, though very briefly, these two things:

First, To consider, what we have upon experience deliver'd in our *Defence against the Learn'd Linus*, touching the Condensation and Rarefaction of the Air, as 'tis exposed to a greater or smaller pressure, without the intervention of either External Heat, or elaborate Engins. For from these Experiments (that may be found in the lately mention'd *Defence**) Eminent Mathematicians have inferr'd, that one can scarce safely put determinate limits to the stupendious Raritv, which the upper part of the Atmosphere, being almost totally un-compress'd by incumbent Particles of Air, may be supposed to have by Nature, un-assisted by Art.

And this is the first of the two things, I above desired to have taken notice of. But the other (which though it be but the second, is much the more considerable) is, to conferre together the smallest extent, to which we have reduc'd it by Condensation, and the greatest, to which we have advanc'd it by Rarefaction, after having taken notice, that, according to the least estimate of any recited in the fore-going Experiments, the extension of the same quantity of Air, is

* Chap. 5. whose Title is, *Two New Experiments touching the Measure of the Force of the Spring of the Air compress'd and dilated.*

as 1 to 2744, or thereabouts, and if, instead of the moderate, we take the greatest Expansion of the Air, being (leaving out the odd hundreds to make the rounder number) as 13000 to 1, when the uncomprest Air was highly rarefied, that number being multiplied by 40, because of the forementioned compression of the Air, will amount to 520000, for the number of times, by which the Air at one time exceeds the same portion of Air at another time: which is a difference of Expansion so great, that I hope it will keep you from thinking the Title of the fore-going Epistle, where the Expansion of the Air is called *admirable*, immodest; especially since I have forborn to mention, what probable arguments might be offer'd, to prove it at least possible, that the industry of men, and perhaps our own, may find means, to make both the Condensation and Rarefaction of the Air to exceed the uttermost, whereto we have yet been able to bring them.

P O S T S C R I P T.

*Touching an Observation to be inserted above, Pag. 16. immediately after the Mark *.*

SInce the writing of this, the Author chanced to find one of the lately mentioned Instruments, of a considerable bigness, which was presumed to have miscarried; and comparing it with a Memorial made, when'twas first compleated, to keep in memory the heights, dimensions, &c. of the inclosed *Mercury* and *Air*; we found, that in about 10 weeks there was not any considerable variation of them; and the little shrinking of the Air, which was discoverable by an attentive Eye, was not such but that it might be probably ascribed to the change of the weather to a far greater coldness, which might be supposed, a little (and it did it but very little) to weaken the Spring of the included Air, and consequently abate of its full resistance to the pressure of the *Mercury* in the longer leg of the Syphon.

